On the Relative Efficiency of Different Methods of Determining Longitudes on Jupiter. By A. Stanley Williams.

The proof of Professor G. W. Hough's further paper on this subject has been read with much interest by me, as it defines his position more clearly with regard to several questions at issue. My remarks on his paper will be considered under four heads— namely, (1) The apparent or accidental errors of the observations ; (2) Systematic error ; (3) The theoretical side of the question ; and (4) Application to the planet *Saturn*.

(1) *The Apparent Errors of the Observations.*

My first paper on this subject, published in the *Monthly Notices*, 1904 March, was entirely confined to the question of the apparent or accidental errors of observation ; all consideration of systematic error, which includes both " personal equation " and what has been termed " variable error," having been purposely excluded. And it has now, I should imagine, been firmly and incontrovertibly established that when a large mass of observations by the micrometric method are compared with a correspondingly large series by the method of transits in exactly the same manner, the apparent errors of the observations are for all practical purposes the same with either method. With both methods the average mean error of an observation is $\pm 2^{m}\cdot 0$ for spots in general, this value being reducible to $\pm 1^{m}\cdot 5$, or less, under exceptionally favourable circumstances.*

There is not very much in Professor Hough's present paper bearing directly on this subject. He states that he finds the mean error of Schmidt's observations to be $\pm 2^{m}\cdot 65$ when they are reduced with a variable increasing rotation period. It is necessary to point out, however, that this result still cannot be accepted as comparable with any of the similar data contained in my writings, for the reason that the variable increasing rotation period is not the one satisfying Schmidt's observations, but that derived from Professor Hough's own measures.

And apparently this is considered to be a fair way of comparing the results obtained by the two observers ! I propose to return to this manner of deriving the apparent errors of observation later on. It should also be remarked that the existence of Schmidt's " constant error," varying with the planet's hour angle, has not in any way been disproved. All that has been done is to show that, as I had anticipated, the apparent errors of Schmidt's

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* To my mind it is much more important to know what are the apparent errors of the observations for spots in general than it is for those made under exceptionally favourable circumstances. The mean error for the five spots with a " large number of observations " (see *Monthly Notices*, vol. lxv. p. 171) observed by Professor Hough with the micrometer is $\pm 2^{m}\cdot 5$, which is distinctly larger than the $\pm 2^{m}\cdot 0$ found for the mean error of an observation by the method of transits for the same class of spots.

uncorrected observations are considerably lessened when the latter are reduced with a variable increasing rotation period, even with one not satisfying the observations in question in the best possible manner.

But in forming an opinion as to the accordance of Schmidt's observations it is important not to overlook his earlier results. His twelve observations of a dark spot in 1862 show a mean error of $\pm 0^{m}\cdot 6$; his six observations of a sharp elbow in the same year one of $\pm 3^{m}\cdot 0$; and his five observations of a white spot in 1866 one of $\pm 1^{m}\cdot 2$.   The mean of these three results is $\pm 1^{m}\cdot 6$, which is practically the same as that given by the best micrometer observations, and considerably smaller than that shown by his latest work, at any rate as regards the uncorrected observations.   And this leads to the question whether there may not be some reason for the apparent errors of these latest obser-vations being perhaps a little large should his " constant error " on a proper discussion prove to have no existence.   No one can possibly have a more profound respect and admiration for Schmidt and his work than the writer.   Nevertheless it must be remembered that these latest observations were made when he was seventy-five years of age, and the work is one in which quickness of perception probably plays an important part.   It has been necessary to go into this matter of Schmidt's observations more deeply than I should have wished, because Professor Hough apparently pins his faith solely on the later observations of this illustrious observer alone.   But surely this is not reasonable now that we have so much additional work by many other well-known skilful observers available for purposes of comparison !

Dr. O. Lohse states, I think,* that the probable error of his central meridian transits is $\pm 2^{m}$, although these, as has already been pointed out, were sometimes made with the help of a micrometer and sometimes without, and therefore do not form a perfectly homogeneous series of observations.   It should be repeated that the mean error of Schmidt's corrected observations is only $\pm 1^{m}\cdot 5$.   The following additional data may be added for comparison with those contained in the list on pp. 433, 434 of the *Monthly Notices* for 1904 March.   Nine observations of the red spot by Mr. W. F. Denning in 1883 show a mean error of $\pm 2^{m}\cdot 3$ (*Astronomical Register*, vol. xxi. p. 172).†   Ten observations by the same observer in 1904-5 October to March give one of $\pm 1^{m}\cdot 2$ (*Observatory*, 1905, p. 188).   Fifteen observations of the red spot hollow by the Rev. T. E. R. Phillips in 1904 give a mean error of $\pm 1^{m}\cdot 5$ (private letter).   Nine observations of the middle of the red spot in 1903-4 by the

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* I have mislaid the part of the Potsdam Observatory *Publications* containing his results, so that I am unable to verify this figure or to work out the actual mean errors of his observations.

† Really rather less, for the ephemeris does not satisfy the observations well.

writer show a mean error of $\pm 1^{m \cdot} 5$; and ten of the following end show one of $\pm 2^{m \cdot} 1$ (*A. N.* 3983). Twenty-five observations (not yet published) of the middle of the red spot by the writer during the past opposition give a mean error $\pm 1^{m \cdot} 5$. The mean of these six additional results is $\pm 1^{m \cdot} 7$, practically the same as that found before.

The real rotation period of a spot is never known, and the only fair way in which we can form an idea of the accidental errors entering into a determination is to compare the observations of any particular observer with an ephemeris satisfying the motion of the spot *according to the observations of that observer*. In almost all astronomical problems we are confronted with the same difficulty, and I believe I am right in saying that it is the universal custom to derive the mean or probable errors in any particular investigation from the accordance of the observations *inter se*. It must be remembered that we are here dealing only with the apparent errors of observation, not with the systematic errors. Yet Professor Hough has derived the apparent errors of the observations made by one observer by comparing them with an ephemeris derived from the observations of another observer. That is, he takes an ephemeris which necessarily satisfies in the best possible way his own observations. A method more advantageous to the micrometrical method and more obviously unfair to the method of transits it would be difficult to imagine.

(2) *Systematic Error.*

That the micrometric method is free from all kinds of systematic error has most certainly never been proved. A few no doubt accidental instances of more or less close agreement with the Marth-Crommelin ephemeris, or with this ephemeris "corrected to conform to the *true* * rotation period," cannot be held to prove it. It would not be difficult to adduce cases of equally satisfactory agreement on behalf of the method of transits. For example, Professor E. E. Barnard's observations of the red spot in 1891 (*Monthly Notices*, vol. lii. p. 12) agree excellently with the ephemeris of Marth—far better, in fact, than do the micrometer observations of 1887 in the instance referred to by Professor Hough. On the other hand, there is no difficulty in selecting cases where the micrometer observations show, from internal evidence, clear signs of being subject to systematic error. Professor Hough admits that he has to "correct" the Marth-Crommelin ephemeris before his observations (of the red spot ?) can be made to conform to the same ; and he also admits that he not infrequently has to use a variable rotation period, sometimes even one with three terms. But in making these admissions it seems to me that he simply cuts the ground from under his feet, for by similarly "correcting"

* *I e.* the rotation period satisfying Professor Hough's observations

the Marth-Crommelin or any other ephemeris,* or by making use of a suitable variable rotation period, the majority of the errors that have been designated as "variable errors" or "cumulative errors" can without difficulty be accounted for or corrected. Hence these admissions really prove that the micrometric method is itself liable to the same systematic errors! Certainly the variations thus corrected in the micrometer observations are in several cases exactly of the same apparent character as those to which the method of transits is subject, and which have been referred to as "variable errors" or "cumulative errors."

Several instances of large variable or systematic errors affecting the micrometric method have been already pointed out. As another example, I would now invite serious attention to the following figures, given in the "Report" of the Director of the Dearborn Observatory for 1885, p. 13, as the variable rotation period deduced for the red spot in 1884–5 :—

					h	m	s
1884 Sept.	25 to Dec.	3	Rot. per. =	9	55	44·0
Dec.	3 „ Feb.	2		9	55	40·1
1885 Feb.	2 „ April	4		9	55	39·1
April	4 „ May	15		9	55	38·7
May	15 „ June	29		9	55	42·0

Since we read that the motion of the spots on *Jupiter* is "smooth, never abrupt," and that abrupt motion is denied, above all in the red spot, it is evident that the remarkable differences here shown cannot be regarded as real. Hence, to quote Professor Hough's own words, slightly modified : † "Here we have apparently a well-established fluctuation in the rotation period of 5·3 seconds (shown by the micrometer measures) which did not exist"! As the red spot is stated to have become more conspicuous, "so much so as to be readily seen with moderate optical power," it is clear that the foregoing remarkable fluctuations cannot be ascribed to the faintness of the spot.

The following statement seems open to argument. "The objects that are observed are usually many millions of miles in area, and presumably have mass. We should not expect any abrupt change in direction or mode of motion in a moving mass." But what reasons have we for presuming that the spots in general have any (considerable) mass? The inference rather is, it seems to me, that the surface density of the planet is probably slight. Moreover, it does not necessarily follow that a given change is due to an actual transference of matter. Many of the known changes on *Jupiter* are possibly only apparent, and attributable rather to the effects produced by the condensation and expansion of vapours and gases, to convection currents, or to comparatively slight changes of brightness or reflecting power. It is

* Including that which satisfies the micrometer observations.
† *Monthly Notices*, vol. lxiv. p. 828.

well known, too, that very sudden and very considerable changes do sometimes occur on *Jupiter*.

Professor Hough's explanation of his discordant observations of a white spot in 1881 hardly seems to me sufficient ; but assuming it to be so, is not this explanation a tacit admission of the validity of my contention as to one way in which " variable error " may arise ?   " The apparent displacement was simply due to the different reference point in making the measures."   This is just what I have been endeavouring to point out is a frequent source of origination of systematic error with the method of transits.*

The position with regard to the whole matter of systematic error may be summed up as follows.   The published micrometer observations and results possess ample internal evidence showing that they are subject to most if not all of the systematic errors to which the method of transits is liable ; besides being, perhaps, also subject to other errors peculiar to the use of the micrometer. At present we have nothing definite to guide us as to the relative size of these errors, nor can we have, so far as I can see, until we possess comparable contemporaneous series of observations with the micrometer by several different observers, such observers working quite independently at distant stations, and using tele-scopes ranging in aperture from, say, 6 inches up to 18 inches. Professor Hough is therefore not justified in treating all, or even the larger part, of the differences found to exist between his micrometer observations and those of other observers, made by the method of transits, as being wholly due to the latter method.

(3) *Theoretical.*

This side of the question has not, I think, been treated quite fairly by Professor Hough.   It is no very difficult matter to compute the shift in seconds of arc of a point near the centre of a planet's disc due to rotation in a given interval of time.   For the present purpose use has been made of a convenient formula published by Professor F. L. O. Wadsworth in the *Observatory*, 1897, p. 369, viz.

$$\varepsilon = \frac{21600}{M} \cdot 0.00029 \cdot \delta$$

where ε is the angular shift in seconds of arc of a point near the centre of a planet's disc per minute, M is the time (in minutes) of axial rotation, and δ the apparent angular semi-diameter (in seconds of arc) of the planet.

It is necessary to decide upon the diameter of the planet to be adopted.   The equatorial diameter of *Jupiter* may vary during the observable period from $50''$ to about $30''$.   The diameter we ought to adopt, however, is that diameter which corresponds to the distance at which the average of the whole number of

* See *Monthly Notices*, vol. lxv. p. 177.

observations is made. Probably at least three-fourths of all observations are made within the period comprised between two and three months preceding opposition and the two or three months following the same. Likewise the great majority of all spots observed are situate within 20° of the planet's equator, so that the reduction in scale due to latitude will, on the average, be small. Consequently 40″ would seem to be a fair diameter to adopt as that at which the observations are made. For short intervals of time, such as five to ten minutes, the shift of a spot near the planet's central meridian due to rotation may be assumed to occur at a uniform rate. The rotation period of the planet we may take to be $9^h 55^m$. Using, then, the above data the average shift of a spot near *Jupiter's* central meridian in one minute of time will be $0''{\cdot}21$; in five minutes it will be $1''{\cdot}05$; and in ten minutes $2''{\cdot}1$.
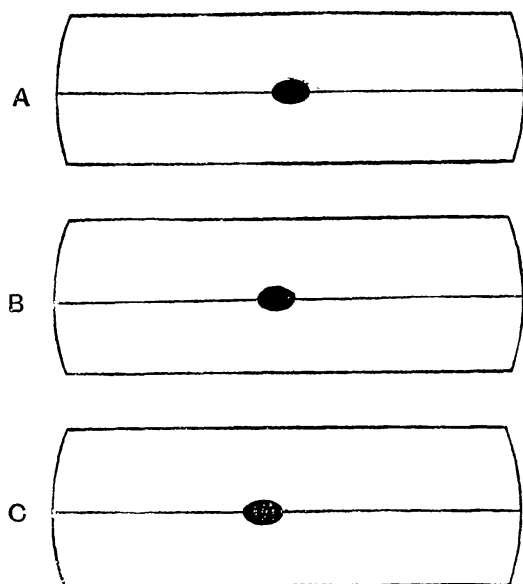
In order to show the effect of this displacement, the three accompanying figures have been drawn as carefully as possible to scale. They represent the rotational displacement of a spot on *Jupiter* three seconds in length. Fig. A shows the spot in mid-transit, fig. B when it is five minutes past transit, and fig. C when it is ten minutes past transit. I believe that most of those who will carefully examine these diagrams will agree with me in concluding : (1) that an experienced observer should be able to observe the transits with an average mean error of $\pm 2^m{\cdot}0$; (2) that under very favourable circumstances such an experienced observer should be able to observe the transits with an average mean error of $\pm 1^m{\cdot}5$, or even less ; (3) that an apparent shift or error of central meridian amounting to as much as five minutes is altogether out of the question, much more so is one of ten minutes or more, as we are asked to believe (it is

hardly likely that it could amount to more than a minute);
(4) that, provided the spot is well defined, there is no reason
why the disc should not be bisected by it with as much accuracy
as by a micrometer wire. If the spot were ill defined or irregular
in shape, then the measures made of it would naturally suffer in
the same degree as the bisection of the disc by the spot itself.*

As corollary to the foregoing it may be concluded that, since
an apparent shift or error of central meridian amounting to much
more than a minute is inadmissible, the known displacements
coming under the description of "variable error," "cumulative
error," "personal equation," and "variable personal equation,"
cannot be accounted for by any such hypothesis. In other words
this means, it seems to me, that the cause of the errors in ques-
tion, or at any rate the larger part of them, must necessarily be
ascribed to something else, namely, to the different manner in
which different observers observe the same planetary markings.†
Consequently micrometer measures by different observers would
necessarily be subject to all and the same kinds of systematic
error. And, further, it follows that all the statements and figures,
so far as they are correct, contained in Professor Hough's papers
respecting the amount and the nature of the systematic errors
affecting the method of transits will apply, and in all probability
with like force, to the micrometric method. That this is the right
way of regarding the matter is shown clearly by Professor
Hough's statement that the "fictitious central meridian chosen
for any group seems to be accidental, since for different spots
observed on the same night the central meridian is different." It
would be difficult to find a more convincing proof that the sys-
tematic errors in question are not ascribable to the choice of a
"fictitious central meridian," but are inherent in or connected
with the appearance of the spots themselves. It is also easy to
imagine the conditions under which the "cumulative error" may

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* The analogy drawn by Professor Hough between the transit of a Jovian
spot and that of a star over an imaginary wire bisecting the field of a transit
instrument, from which the real wires are supposed to be removed, hardly
seems to the writer to be a happy one, because the distance to be bisected
would be many times larger in the latter case than in the former. A better
illustration would be to separate the wires of a micrometer by 40″ and
observe the time when a star is exactly in mid-transit between the two. I
should imagine that an observer, after a little practice, could observe such
times with a very high degree of accuracy. The only advantage the micro-
metric method would seem to have is the power of repetition; but this
appears to be counterbalanced by the uncertainty in setting on the limbs, and
the prejudicial effect caused by placing a wire over a spot.

† Or the same observer at various times and under different circumstances.
I fancy that practically everyone who has ever undertaken the work of
comparing and discussing observations and drawings, not merely of *Jupiter*,
but those of other planets, will be in agreement with the writer as to the
sufficiency of this cause to produce the known variable and other systematic
errors. It must not be forgotten that the appearance of a spot is *largely*
dependent both on the size of the telescope used and on the state of the
seeing, quite apart from any question as to the personal idiosyncrasies of the
observer.

arise, but it is almost inconceivable that an observer could unconsciously select a "fictitious" central meridian for a particular spot differing progressively and in a regular manner with the time from the true central meridian, and yet not applying to neighbouring spots observed on the same nights. But we have still more positive proof, if such were required, that these variable and other systematic errors cannot possibly be due to the selection of a "fictitious" central meridian. For when the transits are observed with the help of a micrometer wire, set to half the measured diameter of the disc, these errors occur just the same.

(4) *Application to Saturn.*

It has been shown that the average angular shift of a point near the central meridian of *Jupiter's* disc per minute is $0''\cdot21$; and also that the average mean error of the micrometer observations is $\pm2^m$, corresponding to a shift of $0''\cdot42$. If we assume the equatorial diameter of *Saturn* to be $18''\cdot6$, and the time of rotation to be 10^h 30^m, we shall get $0''\cdot092$ as the shift per minute of a point on the planet's equator. The spots observed on *Saturn* in 1903 were in a rather high latitude, however. The measures for latitude appear to be somewhat discordant, but probably we shall not be far out if we take it to be $+36°$. The ellipticity of the disc complicates the matter, but multiplying $0''\cdot092$ by the cosine of the latitude we shall get $0''\cdot074$ as the corresponding shift per minute of a spot near the central meridian of *Saturn's* disc in latitude $36°$, $\pm2^{m}\cdot0$, and this will be quite near enough. Since the average mean error expressed in arc of the micrometer measures on *Jupiter* is $\pm0''\cdot42$, a shift of equal amount on *Saturn* in latitude $36°$ will correspond to $5\cdot7$ minutes of time in observing the transit. This, then, is the average mean error that Professor Hough's measures of the white spots on *Saturn* might be expected to have. Under very favourable circumstances a somewhat smaller value might be anticipated, but it will hardly be seriously contended, I should imagine, that the spots on *Saturn* were as well suited for measurement as the great red spot on *Jupiter* was at the time of its greatest plainness. It may be taken, therefore, that $5^m\cdot7$ is a minimum value, and it might be nearly twice as great, judging from the micrometer measures of certain Jovian spots. If, therefore, the few micrometer observations of the spots on *Saturn* show a mean error much smaller than $\pm5^m\cdot7$, such accordance must assuredly be fictitious. This is the case, and such small mean residuals as $\pm2^m\cdot3$ and $\pm0^m\cdot8$ are certainly fictitious, as likewise must be any conclusion regarding the variable motion of one of the spots, based on the scanty given data. It may be mentioned that the mean error of an observation by the method of transits of the principal spot from twenty-eight observations discussed by Professor H. C. Wilson * is $\pm7^m\cdot8$. This is somewhat larger than the theoretical value,

* *Popular Astronomy*, 1903, p. 445.

but this might be expected, seeing that the observations were made by quite a large number of observers, and the spot was certainly more difficult to observe than the average Jovian spot. Mr. Denning has amply explained the reason for some of the larger discordances in the *Monthly Notices*, vol. lxiv. p. 242.

It would be interesting to apply these and some other considerations to the planet *Mars*, and to the case of a fictitious planet of the same size and at the same distance as *Jupiter*, but rotating in half the time that the latter does; but this must be deferred.

Hove: 1905 *June* 5

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Addendum to "Discussion of the Greenwich Observations of the Satellite of Neptune" in Monthly Notices, Vol. LXV., pp. 570–583, by Messrs. Dyson and Edney.

1. It should have been stated that the quantities $s \sin dp$ and ds dealt with in this paper are in the sense "Tabular—Observed," and the resulting values of da, dN, &c., are subtracted from the tabular places to give the results of the observations.

2. On p. 581, although the result is not affected, it would have been more correct to compare the values of N and I found at Greenwich for 1903·1 with the actually observed values found by Dr. Struve for 1890·3, instead of with the values found from the interpolation formula he derived from a discussion of his own and previous observations. The figures are

H. Struve ... 1890·4 $N = 185°\!\cdot\!27$ $I = 119°\!\cdot\!16$⎞
Greenwich ... 1903·1 $N = 187\cdot58$ $I = 117\cdot40$⎠

In 12·7 years $dN = +2°\!\cdot\!31$, $dI = -1°\!\cdot\!76$; and the annual changes of N and I are

$$dN = +0°\!\cdot\!182 \text{ and } dI = -°\!\cdot\!138 \text{ for } 1896\cdot7.$$

Dr. Struve's result for 1874·0 being

$$dN = +0°\!\cdot\!148 \text{ and } dI = -0°\!\cdot\!165.$$

For the mean date 1896·7

$$\psi_2 = 40°\!\cdot\!9 \text{ and } \sin \gamma d\theta = °\!\cdot\!212$$

giving

1896·7 $N = 186°\!\cdot\!44$ $I = 118°\!\cdot\!28$ $\psi_2 = 40°\!\cdot\!9$ $\sin \gamma d\theta = °\!\cdot\!212$

to compare with Dr. Struve's result,

1874·0 $N = 182°\!\cdot\!78$ $I = 121°\!\cdot\!99$ $\psi_1 = 52°\!\cdot\!6$ $\sin \gamma d\theta = °\!\cdot\!208$